by Dr Wolters (the year being 1930). At least, if there are any further instances they are not mentioned by Dr. Wolters. Accordingly, to use the name *aequalicauda* in preference to the widely accepted *ruficauda* would disturb stability and cause confusion.

It seems that Dr Wolters has not understood that, if my application is approved, '*ruficauda* auctorum *sensu* Sharpe, 1879', will become *ruficauda* Swainson, 1838, and hence senior to *aequalicauda* Blyth, 1851. This unused name would thereby become a junior, not a senior synonym, and thus no threat to stability.

SOME COMMENTS ON THE REPORT OF THE COMMITTEE ON TYPIFICATION OF SPECIES OF PROTOZOA. (Z.N.(G.) 185)

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Melville, 1979, presented the report of the committee established by the International Commission on Protozoology to study the problem of typification of protozoal species and enumerated six topics which were discussed. I should like to make some comments which I hope will be useful to scientists considering the implications of that report. Although my examples are drawn largely from the homoxenous coccidia (Apicomplexa: Eimeriidae) they serve to illustrate a wide range of problems in the typification of parasitic protozoans. (Italics used in quotations indicate my own emphases.) The committee's new concept of a hapantotype was further elucidated by Garnham, Bray and Killick-Kendrick, 1979.

2.1. My first comment concerns the committee's definition of a hapantotype (paragraph 5.5), 'individuals taken at one stage in the life cycle and cycled under controlled conditions through the various host species until it is possible to draw off and preserve samples of each stage from a single *strain* which, itself, can continue to exist'. I think that the problem of simultaneously producing a hapantotype consisting of directly related individuals *and* a monospecific strain has not been sufficiently stressed. Joyner, Canning, Long, Rollinson and Williams, 1978, proposed a terminology for populations of coccidia of the genus *Eimeria* at the infrasubspecific level and recommended that '*strains* normally will be established from a *single* oocyst or sporocyst'. It has also been recommended that individual organisms be used to initiate strains of salivarian trypanosomes (Anon., 1978). In other groups of protozoans, a pair of individuals might be needed to initiate a strain, depending on the type of life cycle. It is not necessary to use expensive micromanipulators to isolate individuals. With many protozoans, the medium containing them may simply be diluted progressively until one drop contains one organism.

2.2 It cannot be stressed too strongly that this general principle should be adhered to whenever practicable since, if more than one individual or pair of individuals (whichever appropriate) were used to initiate a strain, a hapantotype derived from it might accidentally consist of more than one species. For example, there has been a great deal of controversy over the question of whether *Eimeria acervulina* Tyzzer, 1929 and *E. mivati* Edgar and Seibold, 1964 constitute the same biological species (Long, 1973; Shirley,

1979). Shirley's 1979 study has shown that these two nominal species are, in fact, valid and that the previous confusion was caused by the use of laboratory 'strains' consisting of mixtures of the two species. This situation could have been avoided by initiating strains with single organisms. There is as much risk of mixed populations arising from the indiscriminate establishment of strains as from the use of heterogeneous cyropreserved material, which was particularly criticized by the committee in paragraph 5.9 of their report. I would suggest that if cloning or some other way of absolutely ensuring the initiation of monospecific strains cannot be carried out, then the designation of a hapantotype should not be attempted and a conventional type should be designated.

3.1 Regarding the use of cryopreserved specimens as a *source* of hapantotypic material (paragraph 5.9), there would seem to be little point in such a procedure if material were readily available directly from a strain being used to establish the biological characteristics of the species but there may well be some situations in which material might have to be cryopreserved pending the designation of a type. However, the use of cryopreserved specimens as *part* of a hapantotype seems to be worthy of consideration. It should be noted that the current amended version of Article 72b(v) of the Code does not preclude the inclusion of *viably* preserved specimens in a hapantotype (see Melville, 1979, p. 207).

3.2 Joyner and Long 1974 described the range of specific characteristics important in the taxonomy of the *Eimeria* species of the fowl, emphasizing the significance of pathogenic effects and immunological specificity. Unfortunately, lesions quickly fade in traditional fixatives and tangible specimens cannot be obtained from the results of cross-immunity tests. Hence, these characteristics cannot be represented in a hapantotype consisting only of *dead* preserved material: *living* specimens need to be available as *stabilates* to provide experimental evidence for differences or similarities between biological characteristics of hapantotypic and other material. Living material is also required to demonstrate a species which can infect different primary hosts and shows variable characteristics in so doing, e.g., *E. dispersa* Tyzzer, 1929 which can infect at least five species of gallinaceous birds (Doran, 1978a, 1978b). A hapantotype made up from nonviably preserved material from one of these hosts might appear very different from another hapantotype of the same biological species infecting a different host: living material would be necessary to carry out host specificity and other tests (as in Doran, 1978b) in order to confirm the conspecificity or otherwise of nominal species. Stabilates removed from cryopreservation for studies to verify an application of a name would not, of course, be returned to the hapantotype; hence replicate stabilates would have to be available. Although it is now known that the species of *Eimeria* infecting chickens can be clearly characterized by isoenzyme techniques (Rollinson, 1975; Shirley, 1975) and the zymograms can be preserved for inclusion in a hapantotype, it should be realized that not all laboratories would have the appropriate facilities and more conventional taxonomic procedures might have to be followed.

3.3 Some of the objections (paragraph 5.9) to the use of frozen material may be answered as follows. There may indeed be variations between the numbers of cells present and their viability and infectivity in each ampoule but there is little significance in this so long as there are some survivors which can initiate infections. Lumsden, 1972, discussing the principles of cryo-

preservation, stated, 'As regards modification of biological characteristics of organisms by the cryopreservation process no clear evidence for this has so far been presented. It appears that cryopreservation is selective in the population in a way *unrelated to its biological variation*'. My own experiences with cryopreserved *Histomonas* and *Eimeria* species bear this out. Regarding mixed populations, these could be avoided if stabilates were established from strains derived as recommended by Joyner *et al.*, 1978.

3.4 The use of frozen material as a viable component of a hapantotype might be criticized on the grounds of the risks associated with the possibility of failure to keep the liquid nitrogen supply topped up. However, there is probably no more risk of this happening than of allowing conventional specimens in spirit to dry out, a not infrequent occurrence in museums. Further information is required on the length of time for which cryopreserved material might remain viable. It should be borne in mind that fixed material of some invertebrate groups may become useless for examination after 50 years or so in spirit.

4.1 The suggestion made in paragraph 5.7 that lesions ('work of an animal' in the sense of the Code) cannot form part of a hapantotype *if they contain no parasites* ('as in the aftermath of *Eimeria necatrix* infections') does not seem logical. None of the examples given in the present Code, p. 154, 'tracks, galls, worm-tubes, borings', necessarily require the presence of the animal originally associated with them. Galls are actually the work of the host in response to parasitic invasion as are parasitic lesions in general. Some lesions are pathognomonic whilst perhaps most are not but they all contribute in some way to the recognition of species. The question of whether lesions can form a useful component of a hapantotype (see paragraph 3.2 of this paper) still must be decided by individual authors.

5.1 Finally, should the concept of a hapantotype be restricted solely to the protozoa? Many phyla include species with several stages of development and, like some protozoa, different biological species might not be distinguishable at every stage of their life cycles. For example the three currently accepted species of *Obelia* (Coelenterata: Hydrozoa) possess indistinguishable medusoid stages. Conversely, in many genera of hydromedusae there is specific diversity of the medusoid generation while the hydroid stage appears uniform (Cornelius, 1975; Russell, 1953). As early as 1864, Allman was stressing the need to define genera and species of Hydrozoa by reference to all stages of the life history. Surely, in the interests of stability of nomenclature, taxonomists working on groups other than Protozoa should be allowed the option of designating hapantotypes when they consider it necessary and practicable. Under the present Code, a worker who, for example, had raised, in the laboratory, planulae, hydroids and medusae of an unknown hydrozoan species would be able to designate only one specimen as the holotype and the rest as paratypes. It would seem more logical to designate all the related stages as a hapantotype.

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